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MENTAL SCIENCE.*

WE have great reason to congratulate ourselves on the progress of psychology, not only in this country but in the world during the last quarter of a century. Not only have students, teachers, text-books, journals, societies, laboratories and monographs increased, and new fields have opened and old ones widened, but our department has been enriched by original contributions that have profoundly modified our views of mind and even of life itself. For the first time in this field American investigators have borne an important and recognized part in advancing man's knowledge of the soul. Among these we take pride even in the presence of our distinguished foreign guests in naming first of all James, who, more than any other American, has occupied and influenced the psychological thought of both experts and students here for a decade, and whose charming personality and style have done most to infect cultivated laymen in all adjacent fields with interest in psychology and to make American thought known and respected abroad; Ladd, to whom we owe the first text on physiological psychology in English and who, more than any other American, illustrates the old tradition of a system of philosophic thought large enough to embrace most of the topics from the laboratory to religion; Münsterberg, who has not only done more than any of his distinguished Teutonic predecessors from Agassiz and Lieber down to make Germany and America know and respect each other, but has been the first to lay the foundations of a new efferent system of thought which harmonized the best in Fichte and Schopenhauer with the choicest results of the laboratory; Titchener with his thorough Eng-

* Address before the Division of Mental Science, International Congress of Arts and Science, Universal Exposition, St. Louis.

lish training, whose ceaseless productivity makes him already in the widening fields he cultivates our American Wundt in a thoroughly and sometimes radically reconstructed and improved edition; Baldwin, the first here to attempt a logic of biology and sociology and evolution that should apply to genetic psychology; Dewey, who, much as he achieved in logic and general psychology, has done perhaps yet more to make these topics fruitful for education; Cattell, pioneer in founding two laboratories, the foremost editor in our ranks, who has boldly grappled the vast problem of individual psychology in a way which, if solved, must make even biography more scientific; in other related fields Royce, Ormond, Howison, Fullerton, Strong, who have done so much to restore the faltering belief in soul, freedom, God and ultimate reality; Cowles, Donaldson, Myer, Hoch, Herrick, each marking advances either in exploring the obscure psychoses of mental aberration, advancing our knowledge of the brain, correlating psychic symptoms and neural and somatic changes, and making the asylum tributary to science—these inadequate references, not to mention my own associates or even the score or two of younger men whose work already gives promise of a future richer in results than the past, and omitting, solely because I am too ignorant to speak of it, the department of sociology, bracketed with ours to-day, but which has also made advances perhaps hardly less signal—these suggest my theme, which is simply a plea for yet more differentiation and specialization between men, departments and institutions, and certain modifications of method in our rapidly widening field.

The idealist who holds that the world is man's concept and that all science is a part of psychology can hardly object to the far more modest claim that it really does properly include logic, ethics, religion, esthetics, epistemology and metaphysics, and those who with Lotze still cling to that dear old tradition of the theoretic life that its supreme joy is to attain the fullest expression of one's own personality in a comprehensive philosophical system, must not carp if one who long since abandoned the youthful hope of attaining this felicity vents his own individuality a little, as, with your kind indulgence, I beg leave to do, excused somewhat by the conviction that all systems, the most meager and the best alike, are only human documents, empirical data, votes, résumés, returns, to be used at last as empirical data for some greater synthesis of the future. If psychology is already far more than a subdepartment of physiology, anthropology and psychiatry, or a subsection in a philosophical system as of old, if we may justly reject for it the place assigned it in the hierarchy of Compte and Spencer as a link between biology and sociology and now base it no less upon the latter than upon the former, and not only claim for it an independence already achieved, but look forward to its ultimate hegemony in all the fields involving man's higher nature, or as being in a word the culmination of humanism, it follows that we must regard all that all of us have so far done as only a prelude, that much of our work must be done over again, that the history of philosophy, instead of being philosophy itself, is to be subordinated as psychological material for a truer and far more comprehensive natural history of mind, and that the best of us are only morning stars which will pale and be forgotten as day advances.

If the germs of soul are as old as life itself and if its types are as distinct and as persistent as those of morphology, then, though we can no more define it than we can life, must we not draw the momentous inference that consciousness alone is a very partial and inadequate organ of experience

and at best only one culminating stage, like a submarine plant's leaves and blossoms that alone reach the surface? Does not experience by its very nature tend to lapse below the threshold of consciousness, and just in proportion as it becomes complete does it not sink beyond the reach of subjective analysis? Nay, more; does it not so strongly tend to become automatic that to become perfect it must lose even the power to be transmitted by instruction, but only by heredity? If so, must we not supplement the methods of internal by those of external observation, subjective by objective, and deductive by empirical researches? Just as history is now studied more as the daily life of the average man and as the play of but half-understood economic racial and telluric forces, and less as the mere records of battles and the acts of kings and courts, so must not psychology more and more center in the study of love, pity, fear, anger, pride, conscience, beauty, love of power and wealth, sympathy and all those social instincts that make our life their sport? Should we not find helpful biological suggestion in the example of Bateson and his growing circle of followers, who would go back to facts baldly recorded to lay broader foundations for their pyramid, instead of steepening its angles to a tower or inverting it, and be content in some fields with a merely descriptive psychology that masses facts, not ignoring those that now seem most trivial, registering reflexes that, perhaps, appear but once in a lifetime, vascular and other somatic resonances that seem meaningless, in the hope that ultimately we may be able to infer something about the psychic states that once animated them and do something to restore the great volume and variety of lost soul and life that mechanism, missing links and extinct species, animal and human, have taken out of the world?

For myself, if I were challenged by some advocate of a psychology without a soul to improvise a working hypothesis of what soul may be like, I might boldly begin by assuming it to have been more potent in the past than it is in the present, but ever tending to vanish as heat, which once made the earth incandescent, does to dissipate itself; as something with which the deathless germ plasm is more instinct than are the somatic organs it evolves, even the brain where it has now taken refuge; as something no less closely related than theology once made the persons of the Trinity to be among themselves with the *nisus formitivus,* whatever that is, which when the world was young and lusty evolved all the products of natural selection, developed and then differentiated hunger and love, adapted flowers and insects to each other, made instinct and inspired all its purposive acts without the aid of any sense of purpose, was shaped by all the forces that have modified life since it began; that domesticated useful and tried to exterminate noxious animals and plants, invented thousands of languages the syntax of some of the lowest of which are the new marvels of philologists; that laid down the lines of the primeval religions and struck out all the unwritten laws and customs of social animals and tribal men, the latter more complex and perfect in many respects, as a recent English blue book on Africa insists, than any that civilized legislators have yet devised. On this view soul life when it was chiefly passion, feeling, impulse, may have been far more dominant over the body and all its processes than now. It was hot, intense, lived out close to the elements, always in sight of the edge of the fierce struggle for survival. It was more life than thought, more collective and racial than individual, shaped the world from within rather than, as science is now learning to do, perhaps, in a derived and secondary way, from without. Everything

was genetic, nothing logical, while as yet no symptom of the great paralyzer, self consciousness, had appeared. This was the great reality which our late developed and senescent, ingrowing intellect has lost and yearns for as old age yearns for its vanished youth. Its traces, fossils, remnants, still abound in our own body and soul and in life about us, but unless we can read our titles clear to this pleroma of life abounding, the psychologist will still have reason to grieve as an exile from his pristine paradise vainly seeking atonement with God, the world and self. Some type of soul life has passed out of the world with every species that became extinct, every vanished tribe, with every child that develops into maturity, with every substitution of self-consciousness and reflection for the naïve, intuitive, spontaneous, and what we call the progress of knowledge is a compound of mingled gains which we keenly feel with losses that we far less keenly realize.

When the intellect, which seems to have been developed late, as a new function of adaptation to the external world, leaves the latter and seeks to introspect its own processes and reflect upon them, it crosses some important pons. This involution is hard, slow and with many stages. Perhaps first are the half-instinctive musings on some memory-content of conduct as affecting pleasure and pain, or upon motor impulses, or the somatic stages that accompany thought are more vividly sensed. Many laboratory experiments directed to other ends have as their best result the revelation of the intricacy of the simplest psychic operations. We find a mazy network of tentative associations and impulses struggling for survival or for emergence into the narrow focus of attention, chaotic irruptions into saner sequences of perception or thought, manifold shades or elements which language is far too clumsy and conventionalized to adequately express, distractions which must be ignored by an act of the will as we ignore all that is in the indirect field of vision. From all this we select out the few and meager factors we want. But if, instead of doing so, we yield to the diverticula and seek to note all that takes place within, we soon feel that we are sane only by a small working majority of our activities, and that underneath the cosmos of habitual sequences and reasoned thought lies a vast and rank chaos of unorganized elements that defy order, analysis or even description. Some are new and some, perhaps, older than history or even than man; some strong and compelling and some so pallid and imperceptible that many experimenters hardly suspect their existence, some congruent and some diametrically opposed to each other. But the tropical sea and jungle are not more rank with life. From all this we realize in a new and deeper sense that conscious mind is only a rather superficial product of gradual and half-unconscious selection, from all this vast seething psychic activity within, of those factors that are practical or most needed for the present conduct of life. Old systems and adjustments to earlier conditions are ever disintegrating and left to lapse and ruin, although they long reverberate in the subliminal field, echo in feeling tones or on occasion have sudden resurgence in automatisms, outbreaks of passion or insistent ideas and impressions, or are injected like dikes into otherwise coherent conduct or thought. The power of survival of these rudimentary organs and processes of the past life of the soul is prodigious. Perhaps they never quite vanish even asymptotically. If the purest science is the completest description of origins and stages of development, psychology may, perhaps, never be complete.

Leaving these, must we not hence infer that the conscious soul we know was evolved solely as an organ to regulate practical life;

that there is no criterion of truth, save its value as a guide to conduct; that the sciences of nature and of mind are, can be and mean only a system of rules for right living and thinking; that to ask what the world and soul are *per se* is an extravasation of the intellect which was kindled only to shed light upon the supreme problem of how to feel and act aright and to which it is subordinate as means to end; that to ask what mind and nature are *per se* and apart from all use by heart and will is paranoiac or a new scholastic entity cult, because the end of science as well as the only real essence of mind is service? Thus the quest of absolute reality must always end in the solipsistic involucre, which is only a new definition of zero, and pure thought purged of will, feeling and sense can not be an object of psychological study, for it does not exist.

Mathematics, which is a formulation of the properties of time and space in the sensory, applies at most only to motion and force in time and space, and its objects are at the bottom, where those of psychology are at the top of the scale of evolution and complexity. From Pythagoras down to Herbart, Fechner, the hedonistic calculus in ethics which the vilest wretch may master without feeling the faintest impulsion to virtue, the Boolean and even common deductive logic which never yet discovered anything, and, indeed, I think, every attempted application of mathematics to psychology, save only for the simple algebraic or other treatment of statistical data, have later proved an illusion if not a mere affectation, and we owe to-day no more to any concept susceptible of mathematical formulation than modern physiology does to the old iatric school that so elaborately treated the bones as levers, the muscles as pulleys, circulation as hydrodynamics, digestion as trituration, and insisted, as Plato did for philosophy, that geometry was the best preparation for the study of medicine. Perhaps no two types of mind have less in common than the mathematical and the psychological or help each other less and may hurt each other more. The former has given us hosts of defunct definitions, categories and dogmas, and has constructed world-bestriding systems by concatenations of the high *a priori* kind in a way that must raise the query in every candid and impartial mind whether in the field of mind the precept 'truth for truth's sake' is not as dangerous as the dictum 'art for art's sake' has proven in its, and whether, beside the old injunction 'physics, beware of metaphysics,' we should not erect the warning 'psychology, beware of mathematics,' and make due purgation of both its methods and its ideals.

Thus, the first and, perhaps, the chief danger to psychology as a science to-day seems to me to be its tendency, as by an iron law, to gravitate to methods that are too abstract, deductive, speculative and affectively exact. Other sciences long since threw off the influence of the old systems, many of which had dominated them, but psychology is still permeated by them. It still feels the charm of the old insolubilities of ultimate reality, of the relation of mind and body, parallelism or interaction, the primacy of feeling or somatic changes, and is dominated more than it knows by interests in the soul's future, by teleology, freedom *versus* necessity, all of which, so far as we see, can never be problems of scientific psychology because they can not be answered. The modern psychologist, too, can be neither materialist, idealist, positivist, dogmatist, gnostic or agnostic, or, rather, is at the same time all of these in some way or degree. Such problems have a large and very important place in the history of philosophic thought. Their culture value as disciplines is very great, but they belong to a stage of mentation now passing

away and doomed perhaps ultimately to become as *ueberwundene Standpuncte,* as those of vortexes, the plenum or vacuum, the Plutonic *versus* the Neptunic theories, have become for science. The ethical bearings of many of these questions once thought so great are rapidly becoming insignificant, but they still bulk large wherever psychologists are dominated by theological interests, or even accept, as far more do, their problems at the hands of metaphysics. Our science is still, like Milton's tawny lion, pawing to get free from the soil in which it is just being born. Many text-books and treatises modulate from the latest science to the oldest speculative surds and speak in two alternating registers, while others evaluate new results by their bearings upon antique problems wrongly put in a pre-scientific age, but made venerable and most significant for history by the accretions of the best and most ingenious thought of ages. Thus, the second danger that besets our work is that it is not sufficiently emancipated from the now conventionalized criteria of past systems of thought, and has not subordinated these as it should to be used not as finalities or solutions, but only as empirical data for larger generalizations that transcend them. But if it is the philosophy of philosophy, it comes to many of these problems not to destroy, but fulfil.

The third source of danger to psychology arises from the theory of knowledge or epistemology. The human soul inherits the result of a vast experience acquired by the race but innate in the individual, but the latter can not validate much of it in his own restricted life. He is so surcharged with paleoatavistic traces, tendencies, instincts, from back, perhaps, to the amphioxus or even the amœba, that he often seems to himself to live and move in a world that is both within and without unrealized, alien and afar. What we inherit is so much better organized than what we acquire, it is so dominant and, perhaps, so unmodifiable and unaccountable, that the world and self seem shadowy, and our unreflecting confidence in these is thus easily shocked out of its poise by Berkeley and Hume till some come to feel that a life so unexplained is hardly worth living. When, in addition to these predisposing causes which for some diatheses may become a neurosis, the thinker leads a pallid, anæmic life in academic isolation from the great, throbbing, struggling world, and in the study devotes himself to passionless contemplation pampered by the second-hand knowledge of life derived from books, it is not strange in a precocious and over-civilized age with more knowledge forced upon the mind than it can digest, that the veil of Maya sometimes settles fold on fold over the soul till it almost feels the panic of the claustraphobiac and must break out and away to find reality or smother. It feels that, like the Holy Grail, removed from the sight of carnal men, it can be sought only by those purged from all defilement of the world of sense, but it must be found and quaffed or the soul be lost to truth. For those paranoiac minds sitting thus in prison, whose constitutional malady is aggravated by the doctrine of the ideality of space, the greatest philosophic delusion of modern times, it is well to have highways of escape opened up out of agnosticism. For many, if not most, too, a touch of it but not too much of it is, perhaps, a necessary part of the complex initiation of youth into its world, but the severer types of this discipline seem more suited to senescent than to adolescent men and races. For the psychologist, however, in pursuit of his legitimate vocation, to be liable to be held up at any time to prove that the soul has a brain or a body, that the self or the objects of sense exist, that other people and animals with similar organs to his own

have similar subjective states, is just as irrelevant and as paralyzing as it would be for the physicist, chemist and astronomer, and any old answer makes just as little real practical difference of any conceivable kind in the one field as in the other. Yet the need of such a cult and all its symptoms, how they came to arise and how very real they sometimes are, and their forms of documentation in both the oriental and the modern world, constitute not only a very important but a fascinating problem for inductive psychology, while the ways of meeting these needs are legitimate and now even pressing questions of the higher individual pedagogy.

Finally, as to the field and methods of scientific psychology, the present speaker feels profoundly limitations that prevent him from rising to the height of the great argument for unification so wisely proposed in the plan of this congress, and can only briefly indicate a view perhaps yet more personal than what has preceded, recognizing that very different ones are held by many if not most of his wiser colleagues. First, no trace of sentiency, even the faintest, down to and perhaps even into the plant world, should be alien to our interest. If in doubt between Wasmann and Forel, on one hand, and the mechanical interpretations of the tropisms and taxies as held by Loeb, Bethe and Uexkull, we should recall and profit by the fate of Descarte's conception that even the higher animals were only automata. In experimenting on these, under the controlled conditions of the laboratory, we should not neglect the observations of the field naturalists nor ignore even the more valuable of the contributions of our agricultural stations, economic zoology, the stock farm and the menagerie, men hunters, etc. Studies here need sympathy as well as controlled conditions. We also want compends of what is known of each of the important animals and birds nearest to man, and to make contact with dynamic or functional biology in its efforts to pass beyond morphology and investigate life, histories, habits and causes of variation, postulating that the manifestations of instinct are just as differentiated and as persistent as those of morphology itself. No philosophic prejudice should make us forget that animals have the same will to live, love of offspring, fear, anger, jealousy, individual attachments, memory, attention, knowledge of locality, home-making instincts and senses that we do. Nor should we deny that empirical methods, whether they have yet done so or not, are quite capable of giving sufficient evidence for the existence of psychic powers as radically different from our own as those claimed for photodermatism or the topochemical sense of the antennæ of ants. Not only, then, might the old maxim, 'Psychologus nemo nisi physiologus' be now also with much propriety reversed, but physiological psychology is now expanding both ways toward a larger biological philosophy, and studies of life and mind will henceforth be more and more inseparable just in proportion as genetic or evolutionary conceptions pervade our field.

Child-study, which began so crudely and has long since silenced many, though not yet all, of the objections raised against it, has already demonstrated its practical value for education, and is acquiring a place of its own in the literature of other departments, especially pathology, philology and criminology, and is beginning to prove itself a key of unsuspected value in unlocking problems connected with the prehistoric development of the race, supplementing studies of the adult mind somewhat as embryology does anatomy and histology. It has not only made new connections between our work and the above departments, but is steadily developing a logic which, though as yet unwritten, is

destined in my own fond belief to become an instrument of great value in reinterpreting the bionomic law of recapitulation, shedding new light upon early developmental stages, and thus giving psychology a genetic perspective which it has so sadly lacked in the past. Students in this field are impregnating insignificant and transient acts, expressions and feeling with new meanings. This work still suffers from the fact that, like the Renaissance, the Reformation and to some extent Darwinism itself, it had to begin outside academic circles, which are now so rapidly opening to it, and develop popular interest and momentum before it could attain scientific methods. It has thus survived and profited by a volume of honest criticism which would have swamped a less vital movement, and that too by many of the very ablest of our craft who did not at first fully understand its scope and value. Now, although we might point with justifiable pride to its books, journals, chairs, its body of results that all accept, we believe that far greater results lie in the near future, and sometimes some of us indulge in dreams of a new dispensation of psychology doctrine with evolution more evolved as its center.

Again, a new alliance is now cemented with the psychological side of anthropology and even ethnology. With almost no academic representation or support, it is our government that has developed a body of scholars that in the study of the Indian have, in the language of another, 'set the world its best example of gathering and recording the myths, customs, rites, occupations and modes of life, thought and feeling of the decadent, yet the most representative of all the races of the stone age.' Psychologists are learning to profit by this work and also to extend their interest to every such record of the sentiments, habits, social organizations and superstitions of primal man. As a naturalist delights in new species, so we more and more both need and desire to profit by every new account that sheds light on how the remotest aborigine thinks, feels and acts, and we do it with a psychic tension and exhilaration as if some great correlation with other allied fields impended. We feel a closer bond with sociology because it, too, is coming rapidly into rapport with anthropology and finding the key to so many of its problems in tribal and other consanguineous forms of early society. The reciprocal suggestiveness of this department with psychogenesis is already beginning to bear fruit.

So in mental and moral alienation we have a few precious and detached studies of psychic symptoms in individuals that are almost classic. The older epoch-making interpretations of epilepsy by Hughlings-Jackson, new views of hysteria, paranoia, a choice fresh little literature on *dementia præcox*, a large collection of records of delusions, hallucinations, automatisms and other phenomena of the borderland between sanity and insanity, gathered at the behoof of an obsolescent hypothesis, but interpreted in a way that has happily called attention to subliminal processes and also the methods of their exploitation by hypnotism. Many of these phenomena are devolutionary; others are normal states magnified by disease as if it were a microscope. Criminology, meanwhile, has shown us feral man in our midst and given a copious anthology of facts about degeneration and perversion, many of which could now be used to make the teaching of practical ethics more interesting and effective. It is high time that mental perversions should be represented by chairs in our medical schools, especially if they are to make headway against quacks and mind-curists, save the profession from some of the tragic experiences just recorded so vividly in the confessions of Veresaeft, teach the medical

student that there is something to be learned outside bacteriology and anatomy, and qualify him to dominate the mind as well as the body of his patient, particularly in a land and age when psychic and nervous complications are more and more involved in diseases. We should not forget the old adage of Hypocrates, 'Godlike is the doctor who is also a philosopher,' which will also bear reversing, and if the psychologist does not study very much anatomy, save the brain and its general structure, the new conception of which he should know, he must give much attention to physiology and have its latest results accessible. At least the psychiatric clinic where nature performs her tragic experiments should always supplement those of the laboratory.

All religions tend to decay, and must be incessantly revived and newly dispensed lest they become raucous and weazened in dogma, conventionalized in rites and rituals, and lose power over individuals, communities and nations, and become divorced from science and life. The multiform symptom-groups of religious pathology are a sad but fascinating chapter only just beginning to be written. Sacrifice and totemism, the faith and prayer states of mind, asceticism, renunciation, miracles of healing, psychology of sects, Sabbath, saints, vows and oaths, the conviction of sin, confession, ecstatic states, worship, the God idea in its many forms, the relations between religion and morals—these and many more old problems, as they begin to be restated in psychological terms, beam with a new light like the cherub faces in old canvases awaiting reincarnation, which they must have if religion is ever to be again made interesting and influential for cultivated men. These themes demand a treatment quite apart from any problems of historicity and should especially be represented in our theological schools, whose pupils ought to have some conception of what the soul they try to save is. Even the so-called philosophy of religion represented by Ritschl and his divergent pupils has not got beyond the restatement of judgments of worth as suggested by Kant's practical critique.

In fine, revolutionizing as the thesis may seem to many, I believe psychology should now be dominantly inductive and practical. Even the old systems, grand as they were, must, as I said, be treated as data of a higher order, whose makers thought they were doing one thing but turned out to have done something very different. Instead of laying bare the constitution of the universe they were only documenting their own souls with unusual fulness for the benefit of the future generalizer. Their work, suggestive as it is, was precocious, and their conclusions premature, and about all of it must be done over again on a larger basis of facts, and our watchword must be not merely back to Kant or even Aristotle, but back to a reexamination of the primitive events of soul life, gathered by the most systematic outer and inner observation, and even from history, literature, experience and wherever psychic life is most voluminous and intense, pain, misery, famine, war, revolutions, shame, revivals, every passional state in which Despine says all vice and crime originate; love, fervid as Dante knew it, crowds, the struggles of the individual soul with besetting sin, which is the original form of dualism as experienced from Paul and Augustine down to poor Weininger, who lately shot himself because he could not overcome the evil within which his almost Manichean system set over against his ideals of goodness. Especially as we advance from the study of sense and intellect to that of the will and feelings, the anæmic thinker, who can realize in his own person so little of the stormy life of man, must seek every possible contact with it. He must live where

he can among animals, children, defectives, the insane, criminals, paupers, saints, sinners, the sick, the well; must know grief and joy—these, as well as the clinic and the laboratory, for here he fronts the bottom facts of the world. Next, he must supplement his at best meager first-hand experience with the proxy experience of others as recorded in books. Psychology lives not merely in the study, but where doubt and belief, sanity and inherited insanity, struggle together; where temptation and conscience wage their wars, in the mob, the cloister; where rage, terror and pity become convulsive and sweep all before them, and where love of the lie usurps that of the truth. Once it was thought that the study of pure should precede that of applied science, but we are now coming to almost reverse this maxim in education. So psychology, especially in our practical age and land, must first study and teach how to live, love, learn, labor; must have something to say to all who reflect on reproduction, disease, health, and thus must first serve man well if it would later rule him wisely. If this view be correct we must abandon many supposed certainties and finalities, and with faith in a future far greater than the past has been, devote ourselves to severe and unremitting toil perhaps for generations; must often practise that hardest of all forms of self restraint in our field—the suspense of judgment—assured that in the end psychology is to become queen of those sciences that deal with man, and reign among all the humanities somewhat as chemistry and physics are coming to do over the material world, with a method, perhaps, sometimes no less exact and certain than these already have. So we shall at last attain a true metaphysics of realities behind sense and feeling which is the necessary crown of all science when it becomes complete.

G. Stanley Hall.

*PRESENT PROBLEMS OF ORGANIC CHEMISTRY.**

There is a strong tendency on the part of some chemists, at the present time, to claim that chemical science in the true sense includes only such portions of our knowledge as can be stated in accurate mathematical terms. One distinguished representative of this school of chemistry has said, 'It is not in the province of science to explain phenomena,' and another has written, 'It is not a part of its ultimate object (*i. e.*, of natural science) to acquire knowledge in regard to mentally conceived existences, such as the atoms of matter, or the particles of luminiferous ether, which are of such a magnitude and character as to lie far beyond the limits of human perception.' I think that nearly all of those now actively engaged in working over the problems of organic chemistry would dissent strongly from these statements. Long experience in dealing with the cumulative, non-mathematical evidence upon which our knowledge of chemical structure is founded has led to a very firm conviction that human knowledge is not bounded by the limits of sense-perception. We are inclined rather to the view that, while there are, undoubtedly, many things which will always remain beyond any direct cognizance of our senses, yet, so far as these have a real existence we may in the end secure, regarding them, very practical and positive knowledge. It is impossible to conceive that those theories with regard to structure which have guided the work of thousands of chemists for the last fifty years do not in some measure express the actual truth with regard to atoms and their relation to each other in organic compounds.

Let us follow, for a few moments, in very brief outline, the steps which have led to the present standpoint. So far as the mat-

* Read at the International Congress of Arts and Science in St. Louis, September 21, 1904.

ters which interest us most are concerned, there was practically no knowledge of organic chemistry before the nineteenth century. The first steps were, of course, the preparation of pure substances and the development of accurate methods of analysis. In both of these fields Liebig was the great master. The formulæ which were calculated were, at first, of little value except to check the accuracy of the analyses and as a simple expression for empirical composition. I need not dwell on the confusion which existed throughout the first half of the century because there was no agreement as to the basis for molecular weights or atomic weights nor upon the large part played by the study of organic compounds in finally clarifying the view of chemists upon these matters. Yet, in spite of this confusion, two discoveries of fundamental importance date from this period: (1) That the empirical composition alone does not fix the nature of a compound, *i. e.*, the fact of isomerism; (2) that certain groups of atoms may remain together in passing from one compound to another through a whole series. The first fact furnishes one of the strongest reasons why an empirical formula for an organic compound is not enough, and the second fact furnishes the most important experimental basis at the foundation of our structural formulæ.

The studies of this period furnished a knowledge of the empirical composition of many natural products and of the products obtained from these by oxidation, reduction and the action of various agents. But while some might, perhaps, be inclined to look upon this mass of empirical knowledge as the most valuable acquisition of that time and to think that the theories in vogue were so imperfect or erroneous as to be of no value, such a view is certainly superficial. There were plenty of chemists in that day, too, who were ready to decry theories which seemed to them worthless, and it is interesting to read to-day what the great Laurent said upon this matter. He wrote in 1837:* "If I could believe that the purpose of my work was only to find a few new compounds or that it would end in my being able to say that there is an atom more or less in this compound or that, I would give it up on the spot. Only the desire of finding an explanation for some phenomena and of proposing some more or less general theories can give me the courage to follow a course in which I have found so little encouragement and where I have met with so many obstacles to overcome." Any one who has followed the story of how the older theories of radicals paved the way for the theory of types and of how the typical formulæ were so easily transformed into structural formulæ when the fact of valence was once grasped, can not fail to see that the larger and fuller view is an outgrowth from the earlier theories. And we must acknowledge that Laurent was right and that the theories upon which he was working were of vastly more importance than the mass of empirical facts which furnished him with their scaffolding.

Do not misunderstand me. There were two theories of radicals at that time—one which devised radicals in the study which should accord with the electrochemical theories held at the time and which did not attempt to secure evidence of their existence from the conduct of the compounds containing them, another which kept in much closer touch with the facts discovered in the laboratory. It was only the latter theory which contributed much to the growth of our knowledge. A theory which can not secure for itself a sound experimental basis is, of course, of only ephemeral value.

These, then, are the steps which have led to our present standpoint in organic chem-

* *Ann. d. Chem.* (Liebig), **22**, 143.

istry: The discovery of isomerism, the discovery of radicals, the older radical theory, the theory of types, the establishment of true molecular weights, the discovery of the fact of valence, the determination of structure.

I think that all workers in organic chemistry will accept the following as a conservative statement of our present knowledge: (1) That in organic compounds, at least, each atom is attached *directly* to only a limited, small number of other atoms; (2) that in the sense of the order of the successive direct attachments the structure of a very large number of compounds is known with a degree of probability that amounts to practical certainty.

This brings me to the task which has been set, an attempt to outline the problems which lie before us in the further development of our science.

In the first place, there is still much to be done to extend our knowledge of compounds found in nature. This field is much less cultivated, relatively, than was the case sixty years ago. There has been good reason for this because of the problems of absorbing interest which have arisen in the preparation and study of new compounds and in the extension of our knowledge of old ones. But there must still remain many compounds to discover among both animal and vegetable products. On this side organic chemistry resembles the descriptive sciences of botany, zoology and mineralogy. And just as botanists think it worth their while to secure as complete a description as possible of the plants to be found on the earth, so it lies in our province to isolate and identify the carbon compounds of the animal and vegetable worlds—with the difference that in our case each compound, new or old, may be the starting point for the preparation of an almost endless number of others. But here most of us recognize that unless a compound has some further interest than that it is new it is not worth the time taken in its preparation. I am afraid, however, as we look over the pages of our journals, there is too much evidence that not every one lives up to this view. Our ever-increasing army of nascent doctors must needs have something to do, and it is so easy to make new compounds and so difficult to find something new of larger scope and really worth the doing.

There still remains much to do in the determination of the structure of compounds which have long been known. The study of a single compound often involves an incredible amount of work. Baeyer worked with indigo for fifteen years before his labors were crowned with a successful synthesis, and twenty years more and the work of very many chemists were needed before the scientific achievement could become a commercial success.

It was nearly twenty-five years after the first structural formula was proposed for camphor before Bredt was fortunate enough to suggest the true arrangement of its atoms, and it was ten years longer and required in all the work of more than fifty chemists before Bredt's suggestion was confirmed by Komppa's beautiful synthesis.

More than thirty formulæ were proposed for camphor, and those who think little of organic chemistry have some reason if they say that we jump at conclusions too hastily and propose too many formulæ that are mere guesses. Some might even say that the last formula isn't worth much, but those who have followed the matter know that step by step we have arrived at an almost positive certainty even in this complex problem.

The final solution of a problem with regard to the structure of a compound of natural origin is not usually considered to have been satisfactorily attained until its synthesis has been effected. Those who

have attempted work of this character know that months or even years of work are frequently spent to obtain the synthesis of a single compound. In spite of the wealth of methods at our command—a wealth so great that it is often very difficult to select between several which are equally unpromising—it is evident that these methods of synthesis need improvement at many points. Not only do we need new and better methods, but many old methods require further study to disclose why they succeed in some cases and fail in others and to secure a fuller knowledge of secondary reactions which often occur. As recent remarkable achievements in this field of synthetic methods may be mentioned the brilliant results obtained by Grignard with magnesium compounds, Bouveault's elegant new solution of the old problem of transforming an acid into the corresponding alcohol and Scheuble's reduction of the amides of bibasic acids to the corresponding glycols.

Work along the lines suggested needs to be done in order to fill out and complete our knowledge in a systematic way, and occasionally work along such lines is rewarded by results of epoch-making significance, as when Gomberg discovered triphenylmethyl in his endeavor to prepare hexaphenylethane. Such work is not likely, however, to greatly advance our insight into the real nature of carbon compounds and we all feel that there are far more fundamental problems which demand attention.

As outlined above, the theories of valence and of structure now universally accepted imply a certain amount of knowledge of the arrangement of atoms in space. So far as the original and fundamental conceptions are concerned, however, this knowledge is quite vague. The much more definite conception proposed by van't Hoff and in a somewhat different manner by Le Bel is, of course, familiar to you all. In discussing any hypothesis it is always important to have clearly before us the facts upon which it is based. As I have already hinted, I believe that the theory of valence and the theory of structure in the sense of a sequence of atoms within the molecule are supported by our knowledge of such a vast accumulation of consistently interrelated phenomena that we are justified in believing that we have positive knowledge with regard to the structure of the molecules of organic compounds. I am as ready as any one to demand that every theory, no matter how old or how universally accepted, shall be continually brought back to the test of agreement with experimental facts, but I am not willing to admit that we may not, in the end, acquire positive knowledge by the process of inductive reasoning.

Assuming, then, the fact of a knowledge of the sequence of atoms in organic compounds, we have this basis for van't Hoff's hypothesis: (1) When four unlike atoms or groups are combined with a single carbon atom optical activity results in such a manner that there may always be found two compounds having identical sequence of the atoms within the molecule, and exactly equal rotary power, but of opposite signs. (2) That when two adjacent carbon atoms are combined each with three unlike groups, two compounds may result which, while optically inactive and having the same sequence of atoms, still differ in physical properties. An illustration of this is found in racemic and mesotartaric acids. (3) Rings containing five and six atoms are formed with especial ease, those containing three, four and seven atoms less readily, and rings containing more than seven atoms are scarcely known. (4) Derivatives of cyclopropane, cyclobutane, cyclopentane and cyclohexane having two substituents combined with different car-

bon atoms often exist in two isomeric forms in which the sequence of the atoms is the same. (5) Derivatives of ethylene often exhibit a similar isomerism.

Assuming as true that we have acquired a knowledge of the sequence of atoms in carbon compounds, the facts which I have enumerated lead almost inevitably to the corollary that the four atoms attached to a given carbon atom are arranged in approximate symmetry around the center of that atom for their position of most stable equilibrium. The relation between this conclusion and the theory of the sequence of atoms in carbon compounds, or what is ordinarily understood as structure, is very similar to the relation between the atomic theory and Avogadro's law. If we accept the atomic theory, there seems to be no rational escape from the acceptance of Avogadro's law. In a similar manner, if we accept the theory of the sequence of atoms in carbon compounds, there seems no reasonable possibility other than that van't Hoff's hypothesis is true in its broad outlines.

I hope I may be pardoned here for a brief digression. I am aware that Franz Wald* believes that he can give a satisfactory explanation of the laws of fixed and multiple proportion and of combining weights without the aid of the atomic theory, and that Professor Ostwald in his recent Faraday lecture† has accepted and expanded the same thought. I will say frankly that their reasoning does not appear to me conclusive. Ostwald defines a chemical individual as 'a body which can form hylotropic phases within a finite range of temperature and pressure,'‡ and deduces from this the fact that a given hylotropic phase must have a fixed composition. He appears to forget that the existence of these hylotropic phases implies that the properties of matter are discontinuous, or, in other words, that there is a finite number of hylotropic bodies, one of the facts for which the atomic theory gives an explanation.

There is another characteristic, too, of a chemical compound which all chemists will agree is at least as important as that it shall consist of a 'hylotropic phase.' This is that the compound must not only have a fixed composition, but this composition must bear a definite relation to those numerical quantities which represent the proportion in which each element of which it is composed always combines with other elements. I need hardly add that these numerical quantities are so deeply seated in the properties of matter that, having adopted a unit, all chemists are absolutely agreed in selecting one and only one such quantity for each of the well-known elements.

In attempting to deduce this law of combining weights Ostwald assumes that three elements form the compounds *AB*, *AC*, *BC* and *ABC*, and adds, 'There shall be but one compound of every [each] kind.' With this assumption, his reasoning may be sound, but I fail to see how it applies when we find ten thousand compounds *ABC* instead of one. The case which he supposes is so far theoretical that I have been unable to find an actual case where the compound *ABC* can be formed, by the union both of *AB* with *C* and of *AC* with *B*.* But I have

* *Ztschr. Phys. Chem.*, **24**, 633, 1897.

† *J. Chem. Soc.* (London), **35**, 506.

‡ *Ibid.*, p. 515.

* It is quite possible that such an illustration may be found, but, in any case, Professor Ostwald's deduction can not be made to apply to those cases in which the compound *ABC* does not exist, nor to those cases where the compound *ABC* can not, even theoretically, be supposed to consist in turn of a known compound *AB* combined with *C* and of another known compound *AC* combined with *B*. Such cases are common because of the fact of valence. In its simplest form the law of combining weights is quite independent of the existence of the compound *ABC* and may be stated thus: If the com-

taken too much time with a matter which is aside from my main purpose. Before leaving this topic I must add, however, that I have used the phrase 'Avogadro's law' advisedly in spite of the fashion set by some chemists of calling it Avogadro's hypothesis.*

I remarked, a few moments ago, that the facts which have been outlined almost compel us to the acceptance of van't Hoff's hypothesis in some form. It is of the utmost importance for us to recognize, however, that we are here at the very confines of our present knowledge and that we must, at every step, bring ourselves back to the rigorous test of experimental fact. In accepting the hypothesis we are not compelled to consider molecules as set pieces of mechanism; on the contrary, there is strong reason for thinking that the positions assumed by the atoms are positions of dynamic and not of static equilibrium. While there have been many speculations in the matter, we have no strong reason for assuming, as yet, any definite shape for the carbon atom, nor even that there are within it definite points of attraction for other atoms. All that seems to be thoroughly established is that for their position of most stable equilibrium the four atoms or groups attached to a given carbon atom are arranged in approximate symmetry around its center. I say *approximate* symmetry because the existence of compounds containing rings of three and four carbon atoms demonstrates that the symmetry is not always absolute, and makes it probable that in cases where the four atoms or groups are unlike the symmetry is also imperfect. So far as I am aware, no fact inconsistent with this fundamental conception is known, while very many facts about optically active and cyclic compounds find in this conception the only satisfactory explanation which has thus far been given. It is true, also, that many facts with regard to optically active compounds indicate that when one group is exchanged for another the exact configuration is often retained, or, in other words, the entering group takes the same position with regard to the other three atoms or groups as was held by the group which was displaced. The manner in which it has been possible to work out, consistently, the complex relations between a considerable number of sugars, gives a very strong experimental basis for this statement. On the other hand, it is well known that such reactions often give racemic mixtures, which indicates that a shifting of groups with regard to a central carbon atom takes place much more easily than the shifting of a group from one carbon atom to another, at

position of two compounds *AB* and *BC* has been determined, the composition of a series of compounds between *A* and *C* can be predicted and a compound which does not belong to this series has never been discovered. A still more general statement of the law, and one which includes, by implication, all of those facts which are used in the selection of atomic weights, is given above. In that form it is more properly called the law of atomic weights.

* Two reasons may be given for this usage. My own view is that we have, by a process of inductive reasoning, acquired such positive knowledge of the existence of atoms and molecules that the expression 'Avogadro's law' is fully justified. But even if we admit the contention of those who think that the atomic theory must always remain an unproved hypothesis, it is possible to frame a definition of the word molecule which would be merely a generalized statement of those empirical facts which lie at the basis of our atomic and molecular theories. Such a generalized, empirical definition must, of course, be very complex but it would not include the concept of discrete particles. Yet it will be still true of these empirically defined molecules that equal volumes of gases contain equal numbers under the same conditions of temperature and pressure. For instance, the term gram-molecule may be considered as a purely empirical generalization and it is true that a gram-molecule of one gas occupies the same volume as a gram-molecule of any other. But this is, in essence, Avogadro's Law.

least in saturated compounds. There are also a number of extremely interesting cases where a reaction gives rise to the optical antipode. Thus Walden has shown* that l-chlorsuccinnic acid is converted by silver oxide into l-malic acid, while potassium hydroxide converts it into the dextrorotatory acid. It is evident that in one case or the other there has been a shifting of the groups. Again Ascham† has shown that when d-camphoric acid is heated with hydrochloric and acetic acids it may be about half converted into l-isocamphoric acid, and that the latter suffers a similar transformation. This case is more complicated, as a 'cis' and 'trans' isomerism of cyclic compounds is involved as well as the optical difference. Not many cases of this character are known, at present, but such cases certainly deserve further study and must be reckoned with in considering the question we have before us. Le Bel‡ has already pointed out the theoretical significance of Waldron's work.

While we may feel that we have comparatively sure ground in the application of the theory of van't Hoff and Le Bel to optically active and to cyclic compounds, the case is quite different when we come to the consideration of what are commonly known as 'double' and 'triple' unions. Professor Michael has done a very great service to chemistry in showing that the supposition of a more or less definite tetrahedral shape for the carbon atom and of 'favored' configurations often leads to conclusions which are at variance with the facts. Philips§ and Blanchard‖ and myself have found a case in which the addition of hydrobromic acid to an unsaturated compound produces an optically active body which evidently has the same configuration as the amino and hydroxy acids from which the unsaturated body is formed by the loss of ammonia or of water. We have here, apparently, a potential asymmetry occasioned by the double union which it is difficult to reconcile with the prevailing conception of such unions. This case is complicated by the presence of a second asymmetric carbon atom in the molecule and is worthy of further study. Rabe and Billmann* have recently described a similar case, but very few instances of this kind are known.

Pfeiffer† has recently suggested a new interpretation of van't Hoff's hypothesis as applied to unsaturated compounds. Pfeiffer assumes that unsaturated compounds retain essentially the same configuration as the saturated compounds, from which they are derived. On this side his interpretation is closely related to the old theory of free valences, which, if I understand him correctly, is favored by Professor Michael. Pfeiffer also brings his interpretation into a close relationship to Werner's theory of inorganic metallic compounds. The most serious objection to the theory is that it supposes the existence either of trivalent carbon atoms or of free valences, in ethylene and its derivatives, an objection which has appeared to most chemists very strong in the past. Pfeiffer points out, it is true, that since the discovery of triphenylmethyl we can no longer deny the possible existence of a trivalent carbon atom‡. It would

* *Ber. d. Chem. Ges.* **32**, 1855 (1899).
† *Ibid.*, **27**, 2004.
‡ *J. Chim. Phys.*, **2**, 344, 1904.
§ *Am. Chem. J.*, **24**, 428.
‖ *Ibid.*, **26**, 281; **27**, 428.

* *Ann. d. Chem.* (Liebig), **332**, 25.
† *Ztschr. Phys. Chem.*, **43**, 40.
‡ The fact that triphenylmethyl exists as a doubled molecule in solution should not, I think, lead us to discard the monomolecular formula for it any more than we consider that acetic acid has, in the ordinary sense of structure, a doubled molecule because it exists as a doubled molecule in solution in benzene or in the state of vapor just above its boiling point, nor because it forms acid salts. In these cases the chemical evidence ap-

seem, however, that the great difference between the intense chemical activity of triphenylmethyl and the comparative inactivity of ethylene demonstrates that, if the latter does in reality have free valences, the fact that there are two such valences reduces the activity of each enormously. The inactivity of carbon monoxide may be significant in this connection.

A more serious objection to Pfeiffer's hypothesis lies in the fact that he supposes so slight a difference in the configuration of fumaric and of racemic acids that it is difficult to see why the former as well as the latter might not be split into a pair of optically active bodies.

We must admit, then, that we have, at present, no satisfactory theory of double and triple unions and that we have here a problem which demands a large amount of further work before it is solved. When the solution is reached we shall probably gain a new insight into the perennial question of the structure of benzene, and our knowledge of tautomerism will cease to be, as it is at present, almost purely empirical. It is possible, perhaps probable, that Thiele's 'conjugated double unions' will contribute toward the solution.

While I have no comprehensive theory with regard to double unions to advance, I will, with a good deal of hesitation, venture to express some thoughts with regard to the combination of atoms in general which have some bearing on this question. We are all familiar with Faraday's law that if a current of electricity is passed through a number of cells filled with solutions of different electrolytes and arranged in series, exactly equivalent amounts of the various components will be liberated at the electrodes in the successive cells. The beautiful experiments of Professor T. W. Richards have demonstrated that we are dealing here with a law which is true for different solvents and over a wide range of temperature; and also that the law is true with a degree of absolute accuracy which is of the same order as the laws of the combination of elements by weight. We are compelled, then, to believe that there is associated with each valence of an ion as it is transported through a solution, or at least as it separates at an electrode, a quantity of electricity which is invariable and independent of the nature of the ion. In other words, we have here a natural electrical unit which can be defined in its relation to atomic weights with a degree of accuracy which seems to be limited only by the refinement of our manipulations.

It is not always recognized as clearly as it should be that this unit quantity of electricity which is associated with one valence of any ion is not a unit of electrical energy. If it were, the same energy would be required to decompose the equivalent quantity of one electrolyte as of every other, which is manifestly not true. While the same current causes the separation of equivalent quantities in the different cells, the differences of potential, and so the amounts of energy required for the separation, vary greatly. It is evident then that when we say that a unit quantity of electricity is associated with each valence of every ion we do not use the term *quantity* in the sense of quantity of electrical energy. Instead of this, when this conception of a unit quantity of electricity is examined it will be seen that it is a conception of something whose properties are those of matter rather than those of energy. The facts appear to be consistent with the idea that the unit quantity of electricity of which we are speaking is of a material nature and you have doubtless already perceived that

pears to be more important and more conclusive than the physical. It is probable that the doubled physical molecule is the result of forces which do not produce a stable structure in the ordinary sense.

I have the theory of electrons in mind. The ingenious experiments of J. J. Thompson have given us considerable reason for thinking that the negative electrons are capable of an independent existence and have also given a probable estimate of their mass, which is small in comparison with the mass of the hydrogen atom.

It has been customary to think of the unit charge of electricity as being involved only in those reactions which occur in solution. If, however, we accept the theory of electrons it is evident that the electrons must be present in the molecule of an electrolyte no matter in what manner it is formed. It is but a step further to the conclusion that the electrons are involved in every combination or separation of atoms and, indeed, may be the chief factor in chemical combination.

Professor Kahlenberg* has shown that a practically instantaneous reaction takes place between hydrochloric acid and copper oleate in a solution in dry benzene, although the solution does not conduct an electric current and there is no evidence of the dissociation of either the copper oleate or of the hydrochloric acid. Professor Kahlenberg points out very justly that there is no apparent difference between these reactions and those which take place in aqueous solutions, where we have much independent evidence of the existence of ions. He draws the conclusion that no ions exist in either case. It would seem that we are equally justified in supposing that a substance not already in the form of ions may separate into them under the influence of a second substance with which it can react.

Some time ago Mr. Lyon and myself† showed that the primary reaction between chlorine and ammonia gives nitrogen trichloride, nitrogen and hydrochloric acid, and that these products are formed in such proportion as to lead to the conclusion that three molecules of ammonia react simultaneously with six molecules of chlorine. It was pointed out at the time that the simplest explanation of this result is to be found in supposing that chlorine atoms separate during the reaction into positive and negative ions, while the ammonia separates partly into positive nitrogen and negative hydrogen and partly into negative nitrogen and positive hydrogen.* This hypothesis has met with some approval,† but has also received the criticism that such a dissociation as is supposed would result in the spontaneous decomposition of ammonia into nitrogen and hydrogen.‡ This criticism loses its force if we suppose that the separation into ions takes place only under the immediate influence of the chlorine with which the ammonia reacts. It has been pointed out by many different authors§ that a separation of atoms from each other must occur either before or at the same time that they enter into combination with other atoms. The only part essentially new in the hypothesis proposed is that this separa-

* *J. Phys. Chem.*, **6**, 1.

† *J. Am. Chem. Soc.*, **23**, 460.

* This was represented graphically thus:

	+ H	− Cl	+ Cl	
≡ N	+ H	− Cl	+ Cl	≡ N
	+ H	− Cl	+ Cl	
	− H	+ Cl	− Cl	+ H
+	− H	+ Cl	− Cl	+ H
+	− H	+ Cl	− Cl	+ H
N				

† Stuglitz, *J. Chem. Soc.*, **23**, 707.

‡ *Ztschr. Phys. Chem.*, **41**, 378.

§ See Erlenmeyer, Jr., *Ann. Chem.* (Liebig), **316**, 50.

tion is into positive and negative parts and that the same atom may be sometimes positive and sometimes negative. The idea of a dissociation which occurs under the influence of a reacting substance appears to be implied in a part of Professor Nef's discussion of methylene dissociation, but it is not always clear whether he has in mind chiefly a dissociation of this sort or one which is independent of the interaction of different compounds.

The thought that the same atom may be at one time positive and at another time negative is related to the older electrochemical theory which supposed water to be positive in acids and negative in bases.

We assume, then, that in every combination of atoms each union involves an attraction between the positive and negative electrons which are associated with the two atoms that unite. In saying this I do not lose sight of the fact that such a thing as attraction *per se* in the sense that one body can influence another at a distance without an intervening medium is, apparently, inconceivable. I think of the attraction as probably caused by some motion of the electrons which enables them to act on each other through the aid of the ether. It is convenient, however, to speak of this effect as an attraction, since our conception of its real nature is, of necessity, very vague. One advantage of the idea that the attraction of the electrons is of a kinetic nature is that we may conceive of the same electron as becoming positive or negative, according to the nature of its motion.

The common conception, at present, is that an atom which has lost an electron becomes positive, while either the electron in its independent existence or the atom to which it is attached becomes negative. So far as I am aware, it has not been pointed out that this view leads to the conclusion that the same atom must, under different conditions, have a different weight. Thus a bivalent copper atom which has lost two electrons must weigh less than a univalent copper atom, which has lost only a single electron. It is true that our methods of determining atomic weights are scarcely accurate enough to detect differences of this order. The suggestion which is made is that the electrons of two atoms which are united have motions which correspond to positive and negative charges, respectively, and that when the atoms separate these motions may be retained, or lost, as in the case of a mercury atom which is uncombined, or that the motions may be reversed. In accordance with the hypothesis outlined above, we must assume that when two atoms separate either one may become positive; dependent partly on their nature, partly on the nature of the reacting substance. The conception here proposed is that of something very similar to the action of the pole of a magnet, which may attract another pole of the opposite kind, or induce the formation of a pole of the opposite kind, or it may reverse the polarity of another magnet.* This is, perhaps, simpler than to suppose the transfer of an electron from one atom to another in those cases where the electrical charges of the atoms are reversed in the ionization. A very accurate determination of the atomic weight of cupric copper as compared with that of cuprous copper might possibly decide between the two hypotheses.

It should be noted that the hypothesis that the electrical charges associated with the atoms are of a kinetic nature, and that these charges may be transferred without gain or loss of matter, is quite independent of the first hypothesis, which is that the

* This is, of course, only an analogy and must not be pressed too far; just as the electrical changes of atoms or ions conduct themselves very differently from those of masses. The latter divide themselves between two bodies in contact; the former may be transferred *completely* from one ion to another.

atoms are ionized when they separate from each other and that the same atom may become either positive or negative.

In following farther the thought of the attraction between electrons as the cause of chemical combination, we must suppose that in addition to the effect of this attraction in holding together the atoms which are immediately attached there is a residual effect upon other atoms within the molecule. This gives a rational explanation of the very great difference in the stability of the union between carbon atoms in different compounds as, for instance, the instability of acetacetic acid in comparison with butyric acid, occasioned by the substitution of an oxygen atom for two hydrogen atoms of the latter. The study of organic compounds has given us a knowledge of a large number of cases of this sort and our text-books contain many empirical rules about them, but there have been few, if any, attempts to give for such facts any rational explanation.

In considering double unions three explanations suggest themselves: (1) We may suppose with Pfeiffer that such unions are in reality single unions and free valences. In this case the presence in adjacent carbon atoms of positive and negative electrons which are uncombined would reduce the attraction of each for the electrons of another molecule, thus explaining why two free valences are so much less active than a single one. (2) We may suppose that the carbon atoms are in reality doubly united, but that, owing to the localization of the electrons in definite parts of the carbon atoms, the four electrons involved can not approach as near to each other as is the case in a single union. This is Baeyer's theory of strain and is much better in accord than is the theory of free valences with the fact that cyclopropane and propylene appear to be about equally unsaturated as evidenced by their heats of combustion and by their conduct toward bromine. On the other hand, it seems to lead logically to conclusions with regard to the addition of bromine to triple unions which Professor Michael has shown are contrary to the facts. (3) Without a condition of strain, we may suppose that the presence of both a positive and a negative electron in each of the atoms united by the double union causes a lessening of the attraction of the electrons. This would result in such a union being less stable than a single union. The second and third views appear, at present, most in accord with the facts—possibly the truth lies in some combination of the two.

Whatever view we may take, it is noteworthy that double unions are usually formed by the loss of a positive and negative atom or group from adjacent carbon atoms, as hydrogen and hydroxyl or hydrogen and bromine. It is also true that in many double unions one of the carbon atoms is more positive than the other, causing the addition of halogen acids in a definite manner which may be predicted in accordance with Michael's 'positive negative law.' Applying this thought to conjugated double unions, we see that of the four atoms involved the two central ones are likely to be positive and negative respectively and neutralize each other's attraction for outside atoms, while an intensified attraction for outside atoms would be found in the exterior atoms. The effect may be analogous to that of the attractive forces of a magnet which exhibit themselves chiefly at the ends.

But I have permitted myself to wander much farther in the field of speculation than was my first intention—farther than is at all profitable, I fear, for these questions furnish, at present, few points for experimental study, and speculations divorced from experiment have usually been profitless. I should be very sorry if what

has been said should give encouragement to such speculations. On the other hand, I have a very firm conviction that we should not be content with rounding out organic chemistry as a descriptive science nor even with adding to the number of empirical rules which enable us to predict certain classes of phenomena. We must, instead, place before ourselves the much higher ideal of gaining a clear insight into the nature of atoms and molecules and of the forces or motions which are the real reason for the phenomena which we study. When we consider the progress which has been made and the knowledge of structure we now possess, which would have appeared sixty years ago to lie beyond the limits of possible acquirement, it is not presumptuous to think that a more complete knowledge of these questions will at some time be gained. This fuller knowledge will take account, too, of many lines of work upon which I have no time to dwell, such as the question of changing atomic volume to which Professors Richards and Traube have directed our attention, and the knowledge of heats of combustion, of molecular refraction and dispersion, of color, viscosity, dielectric constants and other physical properties. The future must give to us a new theory or a development of old ones which shall include all of these phenomena in one comprehensive view.

WILLIAM A. NOYES.
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SCIENTIFIC BOOKS.

MUSEUMS AND MUSEUM APPLIANCES.

The Museum. By L. P. GRATACAP. Reprinted from the *Journal of Applied Microscopy and Laboratory Methods,* Vols. V. and VI.

Bericht über einige Neue Einrichtungen des Königlichen Zoologischen und Anthropologisch-Ethnographischen Museums in Dresden. Von Dr. A. B. MEYER. 4to. Pp. 25; pls. I.–XX.

The first of these is a reprint of Mr. Gratacap's series of papers which appeared in the *Journal of Applied Microscopy* and is a pamphlet of about 100 pages with many illustrations. In book form it would make a good-sized volume and as these articles contain a good résumé of the principles of museum construction and methods of installation, it is a pity that they could not have been issued in such shape. Mr. Gratacap is well qualified to treat of museum matters, and since there is not space to note all the good things he says, it must suffice to discuss a few concerning which there may be room for a difference of opinion. The first is to be found almost at the beginning, where Mr. Gratacap makes a plea for a uniform system in museum methods. The field covered by museums is so vast and the educational features at present so little developed that there is ample scope for diversity in the treatment of museum exhibits, the more that those features that are universally good can only be ascertained by experiment, to say nothing of the fact that the ends sought for may be very different in different museums. To illustrate this we may consider the questions of the display of skeletons and of fossil vertebrates. If the aim is to show the structure and relationship of vertebrates as a whole the two should be combined, the number of specimens should be limited, and fragmentary fossils almost entirely excluded, this being a case where a cast or good drawing is much better than an imperfect specimen, since it is necessary to compare animals in their entirety. If the object is to show the succession of life on the globe, then the fossils should be arranged zoologically under their respective geological periods, so that the visitor can see the successive steps by which the present fauna of the globe has been reached. Still a third arrangement of fossil vertebrates is possible, that adopted by Professor Osborn, of showing by numerous specimens the phylogeny of various groups. If it is desired to show the structure and characters of vertebrates, then skeletons and other anatomical preparations may be placed with mounted animals. No one museum can do all these things and each institution must decide on the plan that best suits its circumstances.

Mr. Gratacap considers the plan and structure of museum buildings, touching particularly on their lighting, and devotes considerable space to case construction, important matters concerning which architects as a rule display lamentable lack of knowledge. It is possible that an architect may, unaided, have designed a good museum case, but if so the reviewer is not aware of the fact, architects' cases being clumsy, poor as to light, and calculated to catch the dust. As to the lighting, it may be said that plate glass should never be used in the windows of museums, but ground glass or luxfer prisms. It is impossible to control the sunlight by the use of curtains and in almost any museum one may at times see the sunlight pouring in and ruining valuable specimens. Diffused light is not only better for the specimens, but better for visitors. Again, windows should not reach to the floor, but be well above it, not merely to give wall space, but because experiment has shown that the best light comes from above, the upper third of a window equalling the lower two thirds in illuminating power. Windows in museums are for the purpose of admitting light, not for giving a view of the surrounding country. Also each window should be fixed so that it may not be opened for the admission of dust and smoke; it is time that museums were ventilated on scientific principles (a few are) and not after the manner of a wigwam. And in the present day of electric lighting it should be an easy matter to overcome the difficulty of dark corners. This leads one to say that it will not be long before more attention is given to lighting exhibition cases from the inside, where the objects displayed can be well illuminated and the exasperating shadows attendant to external lighting avoided. Mr. Ward, of the Milwaukee Museum, has secured extremely good results in this direction, and it has also been used in the large Bass Rock group in the Ipswich Museum, England. Still a word or two more concerning cases, which are treated in considerable detail and are objects about which Mr. Gratacap's wide experience enables him to judge of what should be adopted and what avoided, but it may be added that seven feet is high enough for the average museum case; most large mammals may be accommodated in a case nine feet high and anything over that height is for groups or special exhibits.

The sliding partition, which Mr. Gratacap views with distrust, is neither awkward nor unnecessary, and in the U. S. National Museum the idea has been further elaborated by having the entire bottom of a case running on trunk rollers, and removable. These devices permit the use of large, fixed plate glass panes for the sides of cases and restrict the movable sash to the minimum. A case can not be made that is at once dust-tight and easy of access, and as exhibits are only changed at intervals some concessions may be made. Brackets, pedestals, mountings and labels, each and all claim attention, but the use of many of these depends on the amount of money or size of the working force available. The poor curator may be forced to hang specimens from wires instead of using elaborately finished brass fittings, but, after all, there is a great deal of installation that might be improved with a little thought and care. The average museum errs on the side of endeavoring to show too much; ten good specimens well displayed are worth one hundred badly installed.

Turning now to Dr. Meyer's memoir, which follows the lines of one published about ten years ago, one notes many improvements in cases and methods of installation which Dr. Meyer has found time to devise. He is an ardent advocate of iron cases and, if we remember aright, was the first to work out the details of their construction and bring them into use on any extensive scale.

In the United States he has been followed by Dr. Jayne and Dr. Greenman, of the Wistar Institute, where a plant has been installed for the manufacture of cases for that institution. In the matters of lightness of construction, resistance to changes of climate and safety from fire the iron case is indubitably far in advance of any other. Nevertheless, it has certain drawbacks, including cost and time of construction and, with all due deference to Dr. Meyer, appearance. Pri-

marily a case is for the preservation of specimens, but secondarily it plays an important part in furnishing a museum and this fact should not be lost sight of. A series of huge glass boxes does not form an attractive sight, and at the Wistar Institute it was found necessary to provide the cases with a cornice to mitigate the bareness of their appearance. This criticism applies also to the all-glass case, with the additional remark that we have not yet seen a large glass case through whose joints it was not possible to slip a card.

For storage, library and office use and for purely practical purposes the iron case has no equal, and it is to be hoped that the time may soon come when such cases can be made at a reasonable price and in a reasonable time. From the variety of design shown in Dr. Meyer's paper it would seem that the above conditions have been satisfactorily met in Germany, or broadly speaking, in Europe, for iron cases have been extensively adopted during the past decade. Dr. Meyer gives various views and plans for floor and desk cases, storage cases for insects and eggs, catalogue cases, and bookcases in which a vertical row of shelves may be withdrawn when books are needed or closed flush when not in use. Incidentally Dr. Meyer calls attention to the good results obtained by the use of dyed sawdust as a medium in which to arrange eggs, since this affords a good background and at the same time holds the eggs in any desired place. Naturally if is necessary to be sure that nothing in the dye will affect the eggs. Turnstiles, adjustable trucks for moving cases, swing screens, mountings for skulls and skeletons, are among the things figured and described, all bearing testimony to the thought and time Dr. Meyer has given the subject of museum furniture. F. A. L.

Madreporaria. Parts I. and II. By J. STANLEY GARDINER, M.A. (From 'The Fauna and Geography of the Maldive and Laccadive Archipelagoes,' Vol. II., Part 3, pp. 755–790, Pls. LIX. to LXIV.)

This, the first instalment of Mr. Gardiner's report on the Madreporaria collected by the expedition under his charge to the Maldive and Laccadive archipelagoes, contains: (1) Introduction with notes on variation, (2) the Astræidæ.

I. *Introduction with Notes on Variation.*—The collection contains over 1,000 specimens of dried corals besides a large number in spirit and formalin and a quantity of smaller pieces for comparative purposes. The collections were obtained mainly from (*a*) Minikoi, the most southern bank of the Laccadive Group; (*b*) Goidu, the east island of Goifurfehendu atoll; and (*c*) Hulule, the most southeasternly island of N. Male atoll. An attempt was made to collect as thoroughly as possible—particularly at Minikoi and Hulule—for the comparison of the localities with one another, and specimens were not generally taken from reefs elsewhere. In addition, dredgings yielded a considerable number of specimens, although few hauls were taken on the outer slopes of the reefs.

Mr. Gardiner made a series of observations in order to ascertain how far mode of growth is dependent on light and environmental conditions and had intended to publish these observations, but thinking that the subject had only a limited interest and because of the cost of suitable illustrations, he did not give his observations in full, merely indicating as briefly as possible under each species the variation found in the several colonies due to these causes. It is very much to be regretted that Mr. Gardiner did not present in full the data that he had accumulated on this subject. It is very doubtful if we shall ever obtain a real insight into what constitutes species in corals without studies of the kind which Mr. Gardiner apparently made and without a full presentation of such data. Quite likely considerable experimental work will be necessary in order to ascertain more definitely the influence of certain environmental conditions, such as experiments in planting the young from the same parent colony in different localities where the conditions of growth are different, and observing the resultant growth forms.

Mr. Gardiner divides the variation shown by corals into three classes: (*a*) *Vegetative,* that is the variation that is due to environmental conditions. (*b*) *Continuous,* where a

series from one locality grades into a series from another locality; this kind of variation is exceedingly complex and almost impossible to distinguish from the vegetative. (*c*) *Discontinuous* or *specific* variation. The author says a species in one locality may show *discontinuous* variations in one or more characters, but it by no means follows that the same discontinuous variations will be shown by the same species in a different locality. Work on a collection from a single locality is best calculated to bring out the phenomenon, but one requires a collection from some other locality for comparison.

II. *Astræidæ*.—About 400 of the specimens collected belong to this family. They are divided into 69 species and 21 genera. The author obtained in the Pacific only 12 genera, and Klunzinger obtained from the Red Sea 16 genera (omitting synonyms). This shows that in the Maldive and Laccadive archipelagoes this group of corals is quite rich in both species and genera. In the arrangement of the genera Duncan's classification is followed, and the author correctly says this work is in need of thorough revision.

The following is a list of the genera with the number of species referred to each, and the names of the forms considered new:

Antillia, 1, *A. constricta* Brug., var. *maldivensis* nov.; *Cylicia*, 1; *Tridacophyllia*, 1; *Euphyllia*, 2; ?*Mycetophyllia*, 1; *Mussa*, 1; *Symphyllia*, 1; *Cœloria*, 6, *C. cooperi*, nov., *C. magna*, nov.; *Leptoria*, 1; *Hydnophora*, 4, *H. grandis*, nov., *H. maldivensis*, nov.; *Favia*, 10, *F. adduensis*, nov., *F. laccadivica*, nov., *F. hululensis*, nov., *F. parvimurata*, nov.; *Goniastræa*, 4; *Aphrastræa*, 1; *Orbicella*, 8, *O. minikoiensis*, nov., *O. borradailei*, nov.; *Cyphastræa*, 6; *C. suvadivæ*, nov., *C. maldivensis*, nov.; *Echinopora*, 4; *E. tertia*, nov., *E. magna*, nov.; *Galaxea*, 5; *Stephanocœnia*, 1, *S. maldivensis*, n. sp.; *Acanthastræa*, 1, 2 vars.; *Prionastræa*, 7, *P. suvadivæ*, nov.; *Merulina*, 2.

The reviewer thinks that it is to be regretted that Mr. Gardiner followed the classification of Duncan. Since the publication of the latter author's 'Revision of the Families and Genera of the Madreporaria,' considerable progress has been made in the classification of the Madreporaria and in the correction of the nomenclature. The family name Astræidæ can not be used in corals because no genus of corals can bear that name. A discussion of the name *Astrea* or *Astræa* will be found in the reviewer's 'Eocene and Lower Oligocene Coral Faunas of the United States.'* It has been known for some time that the genera referred to this family by Milne Edwards and Haime would have to be distributed in quite a number of families. Attempts at reclassification have been made by Verrill, Koby, Ogilvie-Gordon, Gregory, Felix and the reviewer. Morphologic proof of the correctness of some of these attempts has recently been published by Professor Duerden in his 'Recent Results on the Morphology and Development of Coral Polyps.'† It seems that it would have been preferable for Mr. Gardiner to have utilized this literature and arranged his specimens more or less in accord with the suggestions contained there; but it must be admitted that the results so far obtained are not always definite, and Mr. Gardiner would have been obliged to supplement by his own work these attempts at reclassification. Knowing the present status of the problem, he evidently preferred following the system of Duncan although he knew it was faulty.

Several of the generic names used by Mr. Gardiner will have to be changed. The name *Tridacophyllia*, de Blainville, should become *Pectinia* Oken.‡ According to Professor Verrill§ the genera *Mussa* and *Symphyllia* must be combined. The genus *Cœloria* is considered separate from *Mæandrina* M. E. & H. (not *Meandrina* Lamarck, 1801). Both Professor Verrill and the reviewer believe that the species hitherto divided into those two genera should be combined into one. This

* 'Monograph U. S. Geological Survey,' Vol. XXXIX., p. 154, 1900.

† 'Smithsonian Miscell. Coll. Quart. Issue,' Vol. XLVII., pp. 93–111, 1904.

‡ Vaughan, 'Some Fossil Corals from the Elevated Reefs of Curacao, Arube and Bonaire,' *Samml. Geolog. Reichs.-Mus. Leiden*, ser. ii., Bd. II., p. 15, 1901.

§ *Trans. Conn. Acad. Sci.*, Vol. XI., p. 177, 1902.

point can scarcely be discussed here, but examinations of large suites of material have convinced the reviewer that the criteria used by Mr. Gardiner for their differentiation are not valid. That is, he says, '*Cœloria* has a true columella, whereas *Mœandrina* has merely trabeculæ extended out from the septa to fill up the axial fossa.' The generic name *Hydnophora* was originally proposed by Fischer v. Waldheim, 1807, for a genus of Paleozoic corals and can not be used for the recent corals to which the name was applied by Milne Edwards and Haime.* *Monticularia* Lamarck, 1816, is available for the *Hydnophora* of Milne Edwards and Haime.

Mr. Gardiner spells such names as *Goniastræa* with the diphthong 'æ.' The original spelling of these names by Milne Edwards and Haime† is with an 'e,' but in their 'Histoire Naturelle des Coralliaires' they change the 'e's' to diphthongs, 'æ's.' The reviewer is of the opinion that the original spelling should be followed instead of the innovation of 1857. The genus *Prionastræa* M. E. and H., should be changed to *Favites* Link.‡

The two criticisms made on Mr. Gardiner's memoir are: (1) He should not have suppressed any data that he possessed regarding the influence of environment on variation. (2) It is regrettable that he has not utilized, and incorporated in his work, recent studies on the classification of the genera treated in his paper, and that his nomenclature is often faulty. However, Mr. Gardiner has given us some of the data regarding environmental influence on variation and his work is thoroughly comprehensible even if he has not utilized modern investigation on the reclassification of these corals. His memoir is decidedly the best contribution that has been made to the so-called 'Astræid' corals of any one area in the Indian Ocean, and its value is very much enhanced by six excellently executed half-tone plates which illustrate 32 out of the 69 species discussed. The criticisms made above may be regarded merely as suggestions for consideration in future work.

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* Trautschold, 'Die Kalkbrüche von Mjatschkowa,' *Nouv. Mém. Soc. Imp. Natur. de Moscou*, t. xiv., p. 38, 1879.

† *Comptes Rend.*, Vol. XXVII., 1848.

‡ Vaughan, 'Fossil Corals from the Elevated Reefs, Curacao, etc.,' p. 21, 1901. Verrill, *Trans. Conn. Acad. Sci.*, Vol. XI., p. 89, 1902.

SCIENTIFIC JOURNALS AND ARTICLES.

THE contents of the October number of the *American Chemical Journal* are as follows:

Contributions from the Chemical Laboratory of Harvard College: 'On Certain Derivatives of the 1,3,5-Triiod-2,4-dinitrobenzol,' by C. Loring Jackson and J. F. Langmaid; 'The Existence of Hydrates in Solutions of Certain Non-Electrolytes and the Non-Existence of Hydrates in Solutions of Organic Acids,' by Harry C. Jones and Frederick H. Getman; 'The Existence of Alcoholates in Solutions of Certain Electrolytes in Alcohol,' by Harry C. Jones and Frederick H. Getman. Contributions from the Sheffield Laboratory of Yale University: CXIX., 'Researches on Pyrimidines: 2-Oxy-4,6-diaminopyrimidine,' by Henry L. Wheeler and George S. Jamieson,' CXX., 'On the Action of Phenylhydrazine on Benzoylpseudoureas: 1,5-Diphenyl-3-aminopyrro-α,β′-diazole Derivatives,' by Treat B. Johnson and George A. Menge'; 'On the Fate of Potassium Myronate in the Animal Organism and Its Hydrolysis by the Ferments of the Liver,' by J. H. Kastle and Eloise Chesley McCaw; 'On the Oxidation of Sulphocyanic Acid and Its Salts by Hydrogen Peroxide,' by J. H. Kastle and Claude Robert Smith. Contributions from the Chemical Laboratory of the Nebraska Wesleyan University: VII., 'The Preparation of Aromatic Nitroso Compounds,' by Frederick J. Alway; VIII., 'The Nitrosocinnamic Acids and Esters,' by Frederick J. Alway and Walter D. Bonner; IX., 'On Certain Nitrogen Compounds,' by Frederick J. Alway and Reuben M. Pinckney; X., 'The Molecular Weights of the Yellow Nitroso Compounds,' by Frederick J. Alway and Ross A. Gortner. Reviews.

The Popular Science Monthly for October may be termed a British Association number, being devoted to the addresses delivered at the last meeting by the presidents of various sections. It opens with 'A Traveler's View of the British Association Meeting,' very pleasantly presented by Henry S. Pritchett and illustrated with views of many of the more noteworthy buildings in Cambridge. Fol-

lowing this are 'Reflections suggested by the New Theory of Matter,' by Arthur James Balfour; a discussion of 'The Mathematical Physics of the Nineteenth Century,' by Horace Lamb; 'Heredity and Evolution,' by William Bateson; 'The Perception of the Force of Gravity by Plants,' by Francis Darwin; 'The Ethnological Work of Lane Fox,' by Henry Balfour; 'On Mountains and Mankind,' by Douglas W. Freshfield; 'Correlation of Reflexes and the Principle of the Common Path,' by C. S. Sherrington; and 'Invention and Discovery,' by Charles A. Parsons. The number contains the index to Vol. LXV.

Bird Lore, for September-October, contains articles on 'President Roosevelt and Bird Protection'; 'A Woodcock at Home,' by E. G. Tabor; 'October Bird Music,' by Earle Stafford; 'King Cole, a Biography,' by Susan M. Morse; the sixth paper on 'The Migration of Warblers,' by W. W. Cooke, and the 'Climatic Variation in Color and Size of Song Sparrows,' by Frank M. Chapman. There are also book reviews and notes, the Audubon Department and an educational leaflet on the 'Screech Owl,' by William Dutcher.

Beginning in January next, a new educational journal, entitled 'The Nature Study Review,' will be published bi-monthly with Dr. M. A. Bigelow, adjunct professor of biology at Teachers College, Columbia University, as managing editor.

DISCUSSION AND CORRESPONDENCE.

THE METRIC SYSTEM.

To the Editor of Science: I wish to object to a statement recently made with regard to the use of old names in countries that have accepted the metric system. It has been said that the common people in continental Europe are not yet fully accustomed to the metric system, and have partly retained the old names in preference to the new ones.

This is absolutely untrue, as far as it refers to Germany. After the official introduction of the metric system in that country, it took a very short time—if not a fortnight, certainly only a few months—to make the 'masses' familiar with it, and about a year after its introduction there was nobody but a few old and decrepit people that had been unable to master it. In fact, there was general rejoicing on account of the riddance from the old and cumbersome system. Indeed, a few of the old words are occasionally used in Germany, but only such that come very close to a metric unit ('Pfund,' 'Maass'), and these are always used in the metric sense, and nobody wants to imply by this that he is opposed to the metric system, or that he is unable to grasp it: it is merely a variety of speech, and distinctly individual or local.

The whole discussion of the metric system carried on recently in Science has been very amusing to the writer, and has furnished stuff for merriment to others. Is it actually the case that the opponents of the metric system believe that the American people are incapable of accomplishing the identical thing that has been done in Germany thirty years ago in a surprisingly short time? Is it actually impossible to obtain correct information about the way it was done there, and about the consequences and incidents connected with this change?

A. E. Ortmann.
Pittsburgh, Pa.

SPECIAL ARTICLES.

IS MATTER TO BE ABOLISHED?

Of late we have been hearing many intimations that matter has had its day. Formerly matter and its properties engaged the attention of the physicist. Later the ether was discovered, and now we are told that the ether and electrons are all that will be left, when the new discoveries have been fully digested. In connection with these statements I wish to lay down a few propositions which may have been overlooked.

It does not appear that electricity has ever been separated from something that has mass.

The propositions that matter has mass, and that a mass of matter has inertia, seem to me to be fundamentally different from each other.

If the inertia of matter can be accounted for as an electromagnetic phenomenon, it may be measured in new units. Instead of being merely a mass phenomenon, it may be referred

to the fundamental units in which all electromagnetic quantities are measured, *i. e.*, the units of mass, length and time.

Formerly the unit of force was referred to the unit of mass only. People talked about a pound of force. Later it was discovered that a force could also be measured in terms of the centimeter, the gram and the second. When that step was taken there was no talk about matter being abolished. Perhaps the present idea is due to the behavior of radium and uranium. When an 'atom' explodes, and exhibits qualities which entitle a few substances to be considered as cranks among substances, we have been thrown off our guard for a moment. We have had all sorts of explosives before. Some of them exploded so slowly that we did not call them explosives at all. When certain crystallized salts lose their form, due to the slow emission of the same emanation that is given off by all animals and plants, we were not greatly disturbed. Nitro-glycerin has long been going to pieces, and giving off more energy per gram per second than any radio-active body yields. But when it was found that these radio-active bodies are going to pieces, and giving off more energy per gram than any other body has been known to give before, we seem to have been induced to suspect all matter of being capable of doing so.

An architect who should learn that the bricks with which he is familiar are not the final elements in his structure would hardly be justified in losing his respect for houses. What does it matter that his bricks contain molecules, which are composed of atoms, which are composed of electrons, and perhaps something else? Does the fact that houses have been known to fall to pieces and give off energy change his estimate of those houses which do not fall? Is he justified in supposing that all houses are really falling, and that their motion will become appreciable if we wait long enough to make the motion so large that we can see it? FRANCIS E. NIPHER.

CURRENT NOTES ON METEOROLOGY.

REPORT OF THE CHIEF OF THE WEATHER BUREAU.

AMONG the most important matters referred to in the administrative portion of the report of the chief of the weather bureau for 1902–3 are the following: In place of the small chalk-plate weather map (11 x 16 ins.), now issued from twenty-three of the weather bureau stations, larger maps (22 x 16 ins.), prepared in the same way, are to be used, if the necessary additional appropriation can be secured. Professor F. H. Bigelow is continuing his studies on the general circulation of the atmosphere and the nature of cyclones and anticyclones, believing that his results 'point unmistakably to a theory which will supersede those heretofore published in meteorological literature.' One of the most important steps which have been taken in the history of the weather bureau is the plan to build up a great center of meteorological research at Mount Weather, on the crest of the Blue Ridge Mountains, about six miles from Bluemont, Va., and the work which it is proposed to do there will, if the plan can be fully carried out, lead to valuable results. Among the investigations which are announced as likely to be undertaken are the exploration of the upper air by means of kites and balloons, the study of numerous problems in solar physics, observations in electricity and magnetism, etc. A new seismograph has been procured and installed in Washington.

MONTHLY WEATHER REVIEW.

The Monthly Weather Review, for May, 1904 (dated July 22), contains the following articles of general interest: 'The Circulation in Cyclones and Anticyclones with Precepts for Forecasting by Auxiliary Charts on the 3,500-foot and the 10,000-foot Planes,' by Professor Frank H. Bigelow; 'The Sensation of Discomfort,' by W. F. Tyler; 'The Promotion of Meteorology,' in which is incorporated a letter from Dr. J. M. Pernter, of Vienna, in regard to a bill introduced into the last congress 'to promote further discovery and research in meteorology'; 'Invariability of our Winter Climate,' by W. B. Stockman. Also the following notes: 'Local Cooperation in Frost Prevention,' 'The Meteorology of Jamaica,' and 'Humming of Telegraph Wires and Poles.' The *Review* also announces the

establishment of a meteorological laboratory at the high school in Chattanooga, Tenn., a citizen, whose name is not given, having provided the means for the purchase of the necessary meteorological apparatus.

GERMAN METEOROLOGICAL SOCIETY.

THE tenth meeting of the Deutsche Meteorologische Gesellschaft was held in Berlin, on April 7–9, 1904, and an account of the proceedings, with abstracts of the papers read, is found in the July number of the *Meteorologische Zeitschrift*. The papers of most general interest were the following: The influence of forests upon climate, by J. Schubert, in which the results obtained recently at the forest experiment station in Eberswalde, Prussia, were discussed. The temperature is found to average lower in the forest than in the open, the maximum cooling effect coming in September, at the 8 A.M. observation. The relative humidity in the forest may exceed that in the open by seven per cent. O. Steffens showed a new form of snow gauge ('ombrograph') in which an oil lamp is included and the snow is melted. P. Polis discussed the distribution of precipitation in cyclones and anticyclones at Breslau and on the Schneekoppe, this being a scheme of investigation which might well be extended. W. Meinardus considered the changes in temperature of the ocean surface off the west coast of Europe, a subject to which he has already given much attention.

NOTES.

THE 1904 volume of the *Annuaire Météorologique* of the Royal Observatory of Belgium contains the usual meteorological summaries, together with the following papers of general interest: H. Arctowski: 'Aperçu des Résultats Météorologiques de l'Hivernage antarctique de la Belgica'; E. Vanderlinden: 'Le Tir contre la Grèle,' a subject which is rapidly assuming a less and less prominent place in the minds of scientific men. The latter paper contains a full bibliography.

The Yearbook of the Department of Agriculture for 1903 (Washington, 1904), contains the following contributions of meteorological interest: James Kenealy, 'Weather Bureau Stations and their Duties'; J. Warren Smith, 'Relation of Precipitation to Yield of Corn'; H. J. Cox, 'Use of Weather Bureau Records in Court.'

Weather Bureau Bulletin No. 34 (1904) is a reprint of an article on climate, written by the chief of the weather bureau for the 'Encyclopedia Americana.' The title is 'Climate: its Physical Basis and Controlling Factors.' In the bibliography one can not help noticing the omission of Hann's 'Handbuch der Klimatologie.'

Smithsonian Miscellaneous Collections, Quarterly Issue, Vol. 2, Part I., 1904, contains 'The Absorption of Water Vapor in the Infra-Red Solar Spectrum,' by F. E. Fowle, Jr.

R. DEC. WARD.

SCIENTIFIC NOTES AND NEWS.

A LARGE number of the eminent European scientific men and scholars, who came to this country to attend the International Congress of Arts and Science, at St. Louis, have been visiting eastern cities and universities. They were received at Washington by President Roosevelt and entertained by Professor Simon Newcomb, the president of the congress; at Cambridge, they were entertained by Professor Münsterberg, vice-president of the congress. Many of them have also visited Johns Hopkins, Pennsylvania, Princeton, Columbia, Yale and other institutions.

DOCTORATES of science have been conferred by the University of Leeds as follows: Lord Kelvin, Lord Rosse, chancellor of the University of Dublin, Sir Isaac Lowthian Bell, Sir James Kitson, M.P., Sir William Henry Broadbent, Sir Arthur W. Rücker, principal of London University, Dr. Thorpe, C.B., director of the government laboratories, Mr. Claudius G. Wheelhouse, LL.D., president of the council of the British Medical Association, Mr. Jonathan Hutchinson, Mr. J. Pridgin Teal, Dr. Hughlings Jackson, Professor Miall, Dr. Tempest Anderson and Professor A. W. Mayo Robson.

A COMMISSION has been appointed by the New York Board of Health to study the causes of pneumonia, with a view to checking the

increasing mortality. It consists of Drs. W. H. Welch and William Osler, of Johns Hopkins, Baltimore; E. G. Janeway, L. E. Holt and T. M. Prudden, of New York; Theobald Smith, of Harvard; Frank S. Billings, of Chicago, and John H. Musser, of Philadelphia.

PROFESSOR RONALD ROSS, of the University of Liverpool, will after his return from Panama, deliver a series of lectures in the Medical Department, of the University of Pennsylvania, on 'Causes and Cure of Malarial Fever.'

SIR CLIFFORD ALLBUTT, regius professor of medicine at Cambridge, Dr. Horatio C. Wood and Dr. John H. Musser made addresses at the opening of the medical department of the University of Pennsylvania, on September 30.

THE RT. HON. JAMES BRYCE, M.P., will deliver the Godkin lectures in Sanders Theater, Harvard University, on October 24, 26, 28, 31, and November 3. He will speak on 'The Study of Popular Governments.' He has accepted the invitation of Columbia University to deliver the first series of lectures upon the Carpentier foundation. The lectures will be given at 4 P.M., on October 10, 12, 14, 17, 19 and 21. Mr. Bryce's general subject will be: 'The Law in its Relations to History.'

PROFESSOR FREDERICK STARR, who holds the chair of anthropology, at the University of Chicago, has given a summer course of five weeks in studying the savage tribes as shown in the various exhibits at the St. Louis Fair.

PROFESSOR A. C. ABBOTT, of the University of Pennsylvania, gave the opening address before the faculty of medicine of McGill University, his subject being 'The Study of Medicine and Recent Advances in Medical Science.'

DR. I. C. WHITE, state geologist of West Virginia, sailed for Brazil, on July 5, where he will examine and report on the coal fields of Rio Grande do Sul.

DR. G. W. LEWIS, of the department of chemistry at Harvard University, has been given a two years' leave of absence to accept a government position in the Philippines.

MR. A. J. COX, instructor in chemistry in Stanford University, has returned after two years spent at the University of Breslau.

MR. ERNST BESSEY, who for three years has been studying abroad, has resumed his duties in the Bureau of Plant Industry at Washington. During Mr. Bessey's stay abroad he has acted in various capacities for the government. Several expeditions have been made by him into southwestern Russia and the Mediterranean coast region in search of new plants and seeds. A number of the promising forage crop seeds secured by him in Turkestan are now being grown and tested under the direction of the Bureau of Plant Industry.

WE learn from *The American Geologist* that at a recent meeting of the Virginia Board of Agriculture an appropriation was voted for a Geological Survey of the mineral resources of Virginia. The survey will be conducted jointly by the State Department of Agriculture and the Virginia Polytechnic Institute. Dr. Thomas L. Watson, professor of geology in the Polytechnic Institute, was appointed geologist in charge of the survey.

DR. A. E. MCDONALD retired on October 1, 1904, from the superintendency of the Manhattan City Hospital, Ward's Island, N. Y.; Dr. J. T. W. Rowe is retained as acting superintendent.

A MEMORIAL tablet to Dr. Jesse Lazear, who died in Cuba in 1900 while investigating the causes of yellow fever, has been unveiled at the new surgical building of the Johns Hopkins Hospital.

MAJOR HENRY F. ALVORD, chief of the dairy division of the United States Department of Agriculture, died at St. Louis on October 1, as the result of a stroke of paralysis.

MR. CLARENCE M. DALY, for sixteen years employed in the experimental department of Mr. Edison's laboratory, has died as a result, it is said, of cancerous growths on his hand caused by work with X-rays.

DR. O. REMBOLD, formerly professor of medicine at Innsbrück, has died at the age of seventy-one years.

MR. RONALD WILLIAM HENRY TURNBULL HUDSON, lecturer in mathematics at the Uni-

versity of Liverpool, with Mr. J. F. Cameron, lecturer of Caius College, Cambridge, was climbing a difficult *couloir,* near Bethesda, in north Wales, on September 20, when he dislodged a mass of rock, and, falling with it, was instantly killed. Mr. Hudson was twenty-eight years of age, and was the son of Professor W. H. H. Hudson, of King's College, London.

THERE will be a civil service examination on November 10 to fill the position of assistant to the agrostologist in the Bureau of Plant Industry at a salary of $1,400. On November 16 examinations will be held for the position of technical assistants in chemistry, pharmacology and zoology in the public health and marine hospital service, with salaries of not less than $1,000.

KING EDWARD has been pleased, upon the recommendation of the secretary for Scotland, to confer the title 'Royal' upon the Edinburgh Museum of Science and Art, and to approve its designation being altered to 'The Royal Scottish Museum.'

FOURTEEN large houses in Bloomsbury, London, have been demolished for the British Museum extension.

THE private herbarium of Dr. John K. Small, consisting of 21,900 sheets, fully representing his collections in the southern United States, has, we learn from the *Botanical Gazette,* been acquired by the Field Columbian Museum.

THE autumn intercollegiate geological excursion for professors, teachers and advanced students will be held at Worcester, October 22. The party will meet on the evening of October 21, at 8 o'clock, in the rooms of the Worcester Natural History Society, 12 State street, when explanation of the work of the next day will be given. The party will start at 8:30 A.M., October 22, from the Bay State House, on a special trolley car; and, after visiting localities in the eastern and northern parts of the city, will return to the hotel for dinner —price, 75 cents. In the afternoon the party will go on the trolley car to the southeastern part of the city, visiting interesting localities there. The works—'The Physical Geography of Southern New England,' by Wm. M. Davis; 'Physical Geography of Worcester,' by J. H. Perry; 'Geology of Worcester,' by J. H. Perry and B. K. Emerson—will make known the points of geological interest in the region. Those proposing to take part in the excursion will please inform Mr. J. H. Perry, 76 Highland street, Worcester, Mass., on or before October 18.

THE Liverpool School of Tropical Medicine proposes to despatch a second yellow fever expedition to the Amazon in view of the necessity of investigating still further this malady. The late Dr. Walter Myers was selected by the school, together with Dr. Herbert Durham, to undertake an expedition to Para to investigate the disease, only a few years ago. Both members of the expedition were attacked by the malady and Dr. Myers died. The expedition wil probably start at the end of the year.

WE learn from *Nature* that extensions made to Millport Marine Biological Station, including a new laboratory, research rooms, a tank-room and a library, the gifts of Mr. James Coats, Jr., of Paisley, were opened on September 27, by Sir John Primrose, lord provost of Glasgow.

WE learn from *Popular Astronomy* that in a circular accompanying the last issue of the *Astronomische Rundschau,* the sale of the instruments and library of Manora Observatory is announced. The telescope is spoken of as one of great excellence. Application should be made to Manora-Sternwarte 'Lussenpiccolo' (Istri).

ACCORDING to the *Experiment Station Record,* it is proposed to hold an International Congress of Agricultural Mechanics at the Universal Exposition of Liège in 1905, under the patronage of the Belgian Government. The organization has, however, not yet been perfected nor the program completed. A. Lonay, director of the provincial school of agricultural mechanics at Mons, is in charge of the preliminary arrangements. Among the topics which have been suggested for the program are instruction in agricultural mechanics, testing stations for agricultural machinery, exhibition and tests of machinery, application of electricity, the traction auto-

mobile, and the establishment of a review of agricultural mechanics.

THE exhibition of the Institute of Hygiene, recently opened in London, differs, according to the *Times*, from any exhibition previously held. It is under the auspices of a medical council and is controlled by medical experts. The intrinsic value of every exhibit is investigated by an examining board. Exhibits only of a high standard of merit are admitted. Every product, article, or appliance passed by the board is 'hall-marked' as to its purity, quality and excellence. Great care is exercised in maintaining the standard of exhibits —alike in the interest of the public health as of the exhibitors. It is recognized that any exhibit passed by the examining board may be used and recommended by medical practitioners. A certificate is granted to exhibitors by the institute, which constitutes a valuable award of merit. The exhibition is intended—(1) to supply medical men with practical information so as to enable them to keep abreast of the times in regard to the value and progress made in the various manufactured products and articles which affect health; (2) to supply matrons and nurses with useful knowledge in regard to those products and appliances required by invalids; (3) to supply the general public, and especially that large and increasing number who take an interest in dietetics, clothing, appliances and articles which are correlated with the health of the person or the home, with authoritative information as to their value and use. The exhibition is situated in Devonshire street, Harley street, in the west end of London and in the heart of the chief medical center of the country. Sample spaces only are allotted to exhibitors, so as to enable a large number and great variety of exhibits to be on view. The exhibition is a permanent one—open daily throughout the year, and is free to the public as well as to the medical profession.

THE *Experiment Station Record* states that the order establishing the soil and fertilizer laboratory in the Bureau of Chemistry, U. S. Department of Agriculture, has been abrogated, and in lieu of this laboratory one to be known as the plant analysis laboratory has been established. The laboratory is charged with the examination of fertilizers and will collaborate in this work with the referees of the Association of Official Agricultural Chemists, and with the investigation of the constitution of plants. It is authorized to collaborate with the Bureau of Plant Industry in the chemical investigation of problems in which the two bureaus are mutually interested. A leather and paper laboratory and a micro-chemical laboratory have also been established in this bureau. The latter is charged with micro-chemical investigations relating to the investigations of the Bureau of Chemistry, and especially the examination of food products with respect to their composition and adulteration. To the leather and paper laboratory will be assigned analyses and investigations relating to tannins and tanning material, all technical problems of a chemical nature relating to the production of leather, chemical and physical examinations of papers with reference to their fitness for use in this and other departments, and problems relating to the production of paper with a view to promoting the agricultural industries connected with the production of the raw materials and to the improvement of the quality of papers made.

UNIVERSITY AND EDUCATIONAL NEWS.

IT is said that Yale University will receive by the will of Mr. Levi Clinton Veits the sum of about $200,000.

THE veterinary department of the University of Pennsylvania has received an anonymous gift of $100,000.

COLUMBIA UNIVERSITY has received a gift of $16,250 from Mr. H. E. Garth for the establishment of a scholarship, and $10,000 from an anonymous donor for the purchase of books.

IT is said that the University of Chicago has received $250,000 for a building for the divinity school.

ACCORDING to the Boston *Transcript*, the Yale University Museum has received from the Royal Museum of Decorative and Industrial Arts at Brussels a large gift of antiquities collected in southern Spain. The collec-

tion represents the transition period between the ages of stone and of metal, and includes many stone and bronze implements, and also pottery. The museum has also received a collection of prehistoric implements collected in the valley of the Susquehanna, and presented by Christopher Wrenn, of Plymouth, Pa.

THE first portion of the $600,000 left by Charles F. Doe to the University of California for a new library building will be available this year; the plans are now being drawn for the building, and ground will be broken in the spring. Like California Hall, the administration building, which is being built by a state appropriation of $250,000, and like the Hearst memorial mining building, which Mrs. Hearst is erecting at a cost of half a million, the new library will be of California granite and fireproof.

THE new surgical building of the Johns Hopkins Hospital was opened last week.

AT the University of Oklahoma a new science hall, first opened for work at the beginning of the present semester, takes the place of the building recently destroyed by fire. The present structure, which is of gray pressed brick, was designed with ample window space in order to secure a maximum of light. It is fitted throughout with electricity, water and gas. The lower floor is occupied temporarily by the department of chemistry, the second floor by biology and the upper floor by geology. An auditorium, used by all departments, is supplied with a modern stereopticon. There are in all more than thirty rooms used as laboratories, offices, lecture and store rooms. Seven men give instruction in the building, and the enrolment in the various departments is much greater than during any previous year.

THE free lecture system of the New York Board of Education under Dr. Henry M. Leipziger will this year be extended so that there will be about 140 lecture centers.

ACCORDING to items in the daily papers, the entrance class at Harvard will number over 600, being its largest freshman class.

THE total attendance at Cornell University exceeds that of any previous year by 206 students. The regularly enrolled students this year number in all 3,300, and the entering class numbers 916, against 815 last September.

DR. CHARLES W. DABNEY will be installed as president of University of Cincinnati on November 16.

THE trustees of Northwestern University have announced the election of Dean Thomas F. Holgate, professor of mathematics, to be acting president of the institution.

DR. HENRY MILTON WHELPLEY, professor of microscopy in the St. Louis College of Pharmacy, has been elected dean of that institution. He has been a teacher in the college since 1884.

DR. C. J. KEYSER has been promoted to a professorship of mathematics at Columbia University.

MESSRS. JOHN FRAZER, Thomas McCutcheon and Walter Taggart have been appointed instructors in chemistry at the University of Pennsylvania.

THE following appointments have been made in the science departments of the University of Maine for the coming year:

H. S. Boardman, C.E. (Maine, 1895), has been promoted from associate professor to professor of civil engineering.

C. P. Weston, C.E. (Maine, 1900), Ph.D. (Columbia, 1904), has been appointed assistant professor of mechanics.

H. R. Willard, B.A. (Dartmouth, 1899), and M.A. (Dartmouth, 1901), instructor in mathematics.

W. K. Ganong, B.S. (Worcester Polytechnic Institute), instructor in electrical engineering.

A. L. Grover, B.C.E. (Maine, 1899), instructor in drawing.

R. K. Morley, B.A. and M.A. (Tufts, 1904), tutor in mathematics.

L. C. Smith, B.S. (Maine, 1904), assistant in chemistry.

Edith M. Patch, B.S. (Minnesota, 1902), Entomologist to the State Experiment Station.

MISS MARY J. HURLBURT has been appointed instructor in physics at Smith College. Miss Harriet W. Bigelow resumes her work in astronomy, after three years' absence.

MR. GEORGE H. CARPENTER, of the Science and Art Museum, Dublin, has been appointed professor of zoology in the Royal College of Science for Ireland.